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Book Review

Review of Davey and Cullen's *Human Operant Conditioning and Behavior Modification*

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Behavior analysts have always been interested in complex human behavior. But the quantity of basic empirical research conducted with human subjects has grown enormously over the past 10 years or so, and the excitement, energy, and enthusiasm of those engaged in this work are evident. Presumably the results of this activity have implications for our basic understanding of human behavior as well as for applications of this understanding. It may be a good time to pause and take stock, which is what a book like *Human Operant Conditioning and Behavior Modification* can encourage us to do.

The book is an edited collection of 11 chapters containing useful, important, intriguing, irritating, and provocative material. Several chapters focus on clinical application. The topics include a review of token economies (Chapter 7 by Kazdin), discussions of neurological problems in clinical populations and their relevance to behavioral intervention (Chapter 6 by Wood), descriptions of relatively recently discovered phenomena from the animal laboratory that might have applied implications (Chapter 4 by Epling and Pierce), and thoughtful analyses of problems confronting the further development of applied behavior analysis (Chapter 2 by Cullen). Other chapters focus on more basic and conceptual issues, including the implications of performance differences between human and nonhuman animals generated by schedules of reinforcement (e.g., Chapter 5 by Perone, Galizio, and Baron, and Chapter 10 by Wearden). Several chapters challenge the view that traditional behavior theory is adequate as a comprehensive account of human behavior (Chapter 1 by Davey, Chapter 3 by Schwartz and Lacey, and Chapter 10 by Wearden). Still other chapters suggest how certain aspects of human behavior, of interest to psychologists who take a nonbehavioral approach, might be studied and analyzed from a behavior analytic perspective. These include the study of social interaction (Chapter 9 by Buskist and Morgan), the quantitative effect of reinforcement (Chapter 11 by Bradshaw and Szabadi), and selective attention (Chapter 8 by Baron, Myerson, and Hale).

Specialists will find the book useful in much the same way that they would find a journal on the topic useful. As would be true for papers in a journal, the chapters vary greatly in scope, approach, content, and purpose. The editors did little by way of developing any larger organi-

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Davey, G., & Cullen, C. (Eds.) (1988). *Human operant conditioning and behavior modification.* Chichester, England: Wiley. pp. ix + 270. (ISBN 0-471-91637-4). This review is the product of a series of seminar discussions among two faculty members (RLS and PSL) and several graduate students that took place during the fall semester, 1988. Each participant made substantial and effective contributions, through short papers and oral comments, to the final product. Thus, the order of authorship after the second is somewhat arbitrary. Our reactions to the book were strongly influenced by our having studied together Skinner's *Verbal Behavior* and Zuriff's *Behaviorism: A Conceptual Reconstruction.* V. Soyars had not been part of those earlier discussions since she just joined our group this past fall (1988). We thank Frank Russell, Ker-Neng Lin, Steven Jones, and Stephanie Sergent for helpful comments. The work was supported by NSF Grant BNS-8519215 to UNCG. Address correspondence to R. L. Shull or P. S. Lawrence, Department of Psychology, UNC–Greensboro, Greensboro, NC 27412.

zation, any coherent framework, that would help readers identify priorities.[1] Davey's introductory chapter, for example, is mainly a plea to take a cognitive approach to human and nonhuman animal behavior rather than a genuine introduction that would alert readers to the important issues and questions in the following chapters. Cullen's chapter (Chapter 2) is far more useful because it asks probing questions about basic and applied behavior analysis and mentions research areas of obvious relevance to the book's title. Yet it too bears little relation to the specific material in subsequent chapters. Adding further to the diversity, at least two of the chapters are reprints of papers published several years earlier. Thus, although readers will find useful material in the various chapters, the book as a whole lacks coherence and integration, and so seems unlikely to be effective either as an introduction to the field or as a summary of its current state.

One could imagine a book where an introduction identified a small set of important questions that are then addressed in each of the separate chapters. For example, what new phenomena, interpretations, controversies, and gaps in our knowledge most urgently call for attention? Is there a specific body of new research and theorizing that has special significance for how we think about human behavior or about behavior generally? How has recent and ongoing research with human and nonhuman animals shaped basic behavior theory? Have applications, based on behavior principles, been consistently more effective in some contexts than in others? Are the failures due to faulty behavior theory or to faulty behavior analyses? Faulty behavior analyses can result from incorrect specification of the relation between the abstract terms of the theory and concrete events. Are there areas of application where behavior analysts could make a valuable contribution but have not done so because of ignorance of important work by nonbehavioral people (e.g., physiological, psychodynamic, and cognitive psychologists)?

The introduction in its present form, however, leaves unclear why some topics were selected for treatment over others. Were these selections made on the basis of some general, systematic understanding of the field, its history, and its future? It seems odd, for example, in the absence of any stated rationale, that work on observing (e.g., Dinsmoor, 1983, 1985) is ignored except for a brief comment in Chapter 10. Dinsmoor's analysis of observing, including the implications of S^- becoming a conditioned negative reinforcer, seems broadly significant. And it seems odd that there is no discussion of the work on stimulus equivalence relations (e.g., Sidman, 1986) and only brief mention (Chapter 2) of analyses of clinical phenomena based on Skinner's system of verbal behavior (e.g., Ferster, 1972; Glenn, 1983; Kohlenberg & Tsai, 1987; Zettle & Hayes, 1982, 1986). In short, we were hoping for more guidance in the form of an overview about what areas are most in need of empirical research, scholarly review, and clear thinking.

Finally, we wonder about the wisdom of including such a diversity of theoretical approaches and languages (from openly cognitive to behavioral) without some clear introductory statement about the nature, similarities, differences, and implications of these different theoretical approaches (cf. Zuriff, 1985, 1986). Diversity may be desirable, but it can lead to confusion in the absence of a systematic treatment.

Davey (Chapter 1) begins the book by suggesting that traditional behavior theory is on the wrong track and that a different approach is needed. The approach he favors is frankly cognitive. He asserts that research with nonhuman animals as well as with humans demands such a view.

[1] For a fine example of introductory and concluding chapters that provide a framework for the content of a multiauthored book, see Catania and Harnad (1988).

The modern conception of any kind of conditioning—whether it be operant or Pavlovian—is as a complex information gathering process which utilizes a variety of cognitive and information pro-

cessing capacities in order to equip the organism with knowledge about important relationships in its environment or between its behavior and its environment.... Furthermore, in humans at any rate, these processes appear to be under the conscious control of the individual. [p. 2]

and,

An important goal of human operant theory is to understand the processes by which humans come to learn about the relationship between their behavior and its consequences, and how they translate this knowledge into behavior. [p. 9]

We suspect that many readers would require far more justification than is provided in order to be convinced of these assertions. A skeptic might wonder, for example, if these are statements about the facts of behavior or about theoretical preferences for interpreting behavior. We appreciate that forceful arguments can be and have been made in favor of the kind of theoretical approach advocated here (e.g., Dickenson, 1980; Killeen, 1984; Roiblat, 1982; Tolman, 1932). But we know of no set of data or line of argumentation showing that we *must* reject a behavior analytic, functional approach in favor of a cognitive/information-processing approach in order to deal effectively with the subject matter (e.g., Catania & Harnad, 1988; Dinsmoor, 1983, 1986; Zuriff, 1985, 1986). Certainly no such carefully developed line of argumentation is presented here. Instead, the case often seems to rely on emotion-evoking rhetoric for its force (cf. Czubaroff, 1988), a few examples of which follow (emphasis added):

The *obsession* with an analysis of operant conditioning in terms of controlling variables meant that the study of the mechanisms (cognitive or otherwise) underlying such learning *was neglected*.... [p. 1]

A second implication of the extrapolation from animals to humans approach has been to *gloss over* the nature of the mechanisms which mediate conditioning in different species. [p. 2]

... and to *extrapolate blindly* without knowledge of underlying mechanisms might frequently lead to erroneous conclusions about the factors which determine human operant performance. [p. 3]

An introductory chapter written along these lines might have been effective if the later chapters had focused specifically on a comparison of behavioral and cognitive approaches, providing sets of data along with rational arguments showing that a behavior analytic approach should be rejected in favor of a cognitive approach. Lacking this, a balanced treatment in the aggregate, equally critical of both approaches, could have been useful. Neither is the case, however.

There is, however, a more general concern throughout the book about the extent to which the principles and concepts of behavior analytic theory are adequate to deal with human behavior. Here again, the reader, disadvantaged by a lack of any general overview, has to extract the relevant dimensions of the more general issue from relatively isolated pieces. One such dimension centers on the extent to which the behavior of humans is similar to and different from that of nonhuman animals and on the interpretation of these similarities and differences. The remainder of our review will elaborate and comment upon this theme.

It is hardly news to nonpsychologists that the behavior of humans differs in certain important respects from that of nonhuman animals. Many of the differences are self-evident to anyone. But what do these differences mean? Do they mean that the science of *human* behavior must develop as a separate field? Or can research with nonhuman animals produce a coherent *system* of principles, concepts, and technical terms that helps us deal more effectively with significant aspects of human behavior, perhaps including the most complex forms of behavior such as verbal behavior? The tradition of behavior theory is that a useful system of wide applicability exists and can continue to develop (e.g., Skinner, 1953, 1957). Behavioral phenomena that seem very different on the surface may come to be understood as similar provided the phenomena are viewed at the appropriate level of abstraction.

Humans have been studied in procedures that bear formal resemblance to procedures used to study the operant behavior of nonhuman animals. Humans press buttons and receive points according to some schedule whereas nonhuman

animals press levers and receive food pellets. The intent of this work is to equate procedures as much as possible and see if the resulting behavior is similar. The consensus is that there are large differences (Chapters 1, 5, 10; but see Chapter 5 for qualifications). But do such differences seriously challenge the generality of behavior principles? They might if it could be asserted that the two situations really were equivalent. The assumption of equivalence, however, seems highly suspect (Chapters 5 and 10). Humans enter our experiments with complex repertoires, verbal and nonverbal, that differ in many ways from those of our nonhuman animal subjects. Who would doubt that these differences can matter? A human can react as a listener, can talk, and can react as a listener to his or her own talk. We can tell our human subjects, for example, that points received during an experiment can later by converted to money. In so doing, we make points function as significant experimental events for our subjects. The histories and current conditions that are responsible for such effects are no doubt complex. One suspects that far more than "points" are involved in the control of our human subjects' behavior. It would be very odd if the effects of extensive social/verbal environments were erased when our human subject entered the experimental chamber. More likely, verbal and other socially related events intrude into any experiment with humans and generate complex effects. Is there any reason why such events would not function as reinforcers, discriminative stimuli, conditioned stimuli, establishing stimuli, instructional stimuli, and as poorly understood "repertoire-altering" stimuli (cf. Michael, 1986)? And could not these social/verbal events operate at both a molecular and molar level, adding further complexity? Is there any good reason to suppose that a button-press for a human is equivalent as a response unit to a keypeck for a pigeon? The operant as a unit can function at very different levels of molarity, depending on a variety of conditions and histories (Marr, 1979; Skinner, 1957; Thompson & Zeiler, 1986). Given these considerations, should one not be more surprised when the performances of human and nonhuman subjects appear similar than when they appear different under complex conditions? Indeed, when the performances appear similar, might one not fairly wonder whether the similar outcomes in fact imply similar controlling relationships?

Many of these differences and complexities are identified in several chapters, most notably those by Wearden (Chapter 10) and by Perone, Galizio, and Baron (Chapter 5). Interestingly, their conclusions are quite different. Wearden's conclusion, like Davey's (Chapter 1), is that a cognitive approach to human behavior is called for, although the reasoning that led him to this conclusion is not described in sufficient detail to evaluate. Again, it is not at all clear to us why these kinds of complexities, plausibly identified, favor a theoretical approach based on inferred cognitive processes and an information-processing metaphor over one based on environment-behavior relations. Critics (e.g., Chapters 1, 3, and 10) assert that we have not developed a compelling, rigorous, effective behavioral analysis of these complex phenomena. We agree (cf. also Catania, 1986; Michael, 1986; Shimoff & Catania, 1988). Yet we remain skeptical that other theoretical *approaches* offer greater promise of a rigorous, effective analysis. Perone, Galizio, and Baron (Chapter 5) see no reason to abandon the behavioral approach. Nor have others who have recognized the complexities resulting from speaking and listening repertoires (e.g., talking to oneself) (e.g., Ferster, 1972; Goldiamond, 1962; Greenspoon & Brownstein, 1967; Hayes, 1986; Shimoff & Catania, 1988; Skinner, 1953, 1957, 1969; Verplanck, 1962).

Given the complexities arising from speaking and listening repertoires, the systematic implications of the human-nonhuman performance differences under schedules of reinforcement are going to be enormously difficult to determine (cf. Chapter 5). One might wonder, then, exactly what purposes are served by studying the performance of humans on

schedules of point production in tightly controlled laboratory arrangements. Fundamental controlling relationships are revealed with *analytic preparations* chosen for their convenience relative to the question and the experimental task at hand. The *principles* are intended to apply at a more abstract level of description. For example, the strengthening effects of reinforcement are most easily studied with *preparations* where the response and reinforcer are both brief, discrete events, where preexperimental histories have little systematic effect, and where consequences of responding exert little discriminative control. The *principle* of reinforcement, however, is assumed to apply far more broadly—to situations, for example, where the relevant events are not so readily apparent.

If the purpose is to investigate the strengthening effects of reinforcement, the "human Skinner Box" might be an extremely poor analytic preparation. The combination of variables, given the elaborate repertoires and social/verbal histories, might simply be too complex to permit straightforward relationships to emerge. One could push onward in an effort to determine precisely the basis of human-nonhuman differences on schedules, recognizing, as Perone, Galizio, and Baron (Chapter 5) do, that such performances are multiply and complexly determined. Or one could take a different approach, focusing on basic controlling relationships and devising appropriate preparations to investigate those relationships. Perhaps the "human Skinner Box" is a highly suitable preparation for studying complex forms of stimulus control (Sidman, 1986) and instructional control (e.g., Catania, Matthews, & Shimoff, 1982), even if it is not well suited for studying the strengthening effects of reinforcement. But perhaps there are better preparations for studying these important phenomena.

The study of instructional control, and especially self-instructions, is not going to be an easy matter at all, however. Long ago behaviorists rejected introspection as an effective scientific method, mainly on pragmatic grounds (Zuriff, 1985). Consequently, behavior analysts tend not to be experienced in dealing with verbal descriptions of performance in experimental settings, and some of our work may suffer from insensitivity to methodological issues. Those embarking on such work, therefore, would be well advised to read Perone, Galizio, and Baron's (Chapter 5) careful discussion of a number of methodological issues that arise in this work.

Behavioral differences also can emerge as a function of physiological differences. Wood (Chapter 6) discusses these in the context of clinical neuropathology. Often it is found that behavior principles appear not to work with certain clinical populations. The tendency, then, may be to question the generality of the principles. An alternative, implied by Wood's discussion, is that differences in organismic conditions might be viewed as altering aspects of the controlling relations that we usually take for granted. For example, conditioning might occur to unusual and unsuspected classes of stimuli. Stimulus control by internal events might be disrupted in particular ways. Particular training in "attention" (or observing) can have profound meliorative effects, as Wood demonstrates. Here, clearly, is an area where people trained in clinical neuropsychology and people trained in behavior analysis should be able to work productively together. The facts of neurological effects do not demand that we abandon our behavioral approach in favor of an internally based interpretative approach.

Where some of the chapters stress differences in performance between human and nonhuman animals under formally similar circumstances, others (Chapters 3, 4, 7, 8, 9, and 11) stress similarities. For example, several chapters (Chapters 4, 8, and 11) provide data and analyses showing that Herrnstein's equation relating the rate of a response to its relative reinforcement can describe the rate of button pressing by humans under schedules of point production. Herrnstein's equation emphasizes that the rate of a response is influenced not only by its own rate of reinforcement but also by the total

reinforcement in the context from all sources. Herrnstein's equation can serve as a reminder to practitioners of the important role played by alternative sources of reinforcement in the maintenance of a target response. Traditional statements of the reinforcement principle do not so forcefully draw attention to the context of reinforcement. Bradshaw and Szabadi (Chapter 11) show, further, how the values of the constants in Herrnstein's equation might be used to distinguish classes of clinical pathology. Baron, Myerson, and Hale (Chapter 8) show how an application of Herrnstein's equation can contribute to an analysis of attentional effects generated from a cognitively oriented research program. Their treatment is reminiscent of a signal-detection analysis. Following Catania's earlier distinction (Catania, 1973), Baron, Myerson, and Hale distinguish between structural and functional questions. Cognitive work on attention has tended to focus on issues of structure; behavioral work on reinforcement has tended to stress functional relations. Regardless of interpretative language, the two kinds of questions may be viewed as important and complementary. Their work suggests one way that data generated from both behavioral and cognitive traditions might be brought to bear on the analysis of important behavioral phenomena.

Epling and Pierce (Chapter 4) review several lines of work conducted with nonhuman animals that might have applied implications. In addition to research associated with Herrnstein's equation (the matching law), they identify economic (or regulatory) approaches to schedule effects and schedule-induced behavior as significant. They suggest, for example, that some clinically interesting behavior that has been difficult to understand might be understood as examples of adjunctive behavior. Such behavior is induced as a by-product of a reinforcement schedule for some other response. Buskist and Morgan (Chapter 9) show that under certain conditions, human cooperation and competition vary as a function of operant contingencies in a way that would be expected from basic principles. They relate these results to phenomena described by social psychologists. The possibilities of connections between behavior analytic methods and conceptions and work in social psychology is exciting.

These kinds of similarities are surely provocative and suggestive of the broad applicability of behavior principles. Yet here, too, caution is in order (Chapters 1, 2, 3, 5, 10).

Schwartz and Lacey's concern (Chapter 3) is that the similarities, and indeed the familiar behavior principles themselves, might be the product of highly constraining experimental preparations. They argue that processes that are acknowledged by such everyday terms as *intentions, goals, creativity, beliefs,* and *purposes* operate in natural environments but are suppressed in highly constraining environments. Thus, for example, reinforcement emerges as a prepotent process only because experimental conditions inhibit the effects of the other processes. Reinforcement and other familiar behavioral processes will be prepotent in human behavior, Schwartz and Lacey argue, only in environments that are constraining in much the same way as the Skinner Box is, the factory being one such instance. An implication of their argument is that behavior theory does not describe the behavior of humans (or nonhuman animals) generally but only in particular constraining environments. We humans can construct environments (e.g., schools, clinical institutions, etc.) that are constraining so that behavior conforms to behavior theory. Or we can construct environments that allow the full range of behavior potentials.

Schwartz and Lacey's arguments seem highly dubious, however (see also Brownstein & Shull, 1985). Again, they seem to be taking characteristics of analytic preparations to be characteristics of behavior principles. We may agree that the work of artisans is not easily conceptualized as repetition of brief pieces of behavior reinforced by arbitrarily connected, discrete, obvious consequences. But that does not mean that reinforce-

ment and other familiar behavioral processes are not operating in complex combinations. Responses, stimuli, and reinforcers are not normally brief, discrete, obvious, independent events. Special pains have to be taken to construct analytic preparations with those properties. Many behavior analysts would see many complex contingencies operating in the kinds of situations that Schwartz and Lacey describe as being outside the purview of a behavioral interpretation. Also, it may be worth emphasizing that an appropriate behavior analysis recognizes other classes of controlling relations than reinforcement. And the stimulus and response classes can vary enormously in complexity. Readers who have studied material such as Skinner's *Verbal Behavior* (1957), Zuriff's (1985, 1986) analysis of behaviorism, or Ferster's (1972) analysis of clinical phenomena will find Schwartz and Lacey's characterization of behavior theory inaccurate and their arguments unconvincing. (Furthermore, as elaborated above and below, it is doubtful that the behavior of workers in a factory exemplifies the straightforward strengthening effect of response-contingent reinforcement. The social/verbal contingencies are much too complex for that interpretation to be plausible.) Their chapter does, however, usefully raise questions about the kinds of evidence that would demonstrate the comprehensiveness of behavior theory.

A second kind of concern about the interpretation of similarities is expressed in several chapters (1, 5, and 10). Similar-appearing outcomes can result from very different controlling relationships. Consequently, similarity in the form of behavior, or even in the form of functional relations under restricted conditions, does not demonstrate that the controlling relations are similar. In several chapters this point is presented as an attack on behavior analysis as an approach. Yet behavior analysts are very familiar with the point: behavior is classed in terms of functional relations rather than in terms of form or topography (e.g., Skinner, 1957; Thompson & Zeiler, 1986; Zuriff, 1985). The distinction between contingency-shaped and instructed (or rule-governed) behavior (e.g., Skinner, 1969) is one familiar example. The former may exemplify the direct effects of reinforcement in strengthening a particular class of responses; in the latter case the instructions are functioning as antecedent stimuli of some sort that evoke particular responses due, presumably, to a complex history relative to instructions. The behavior of adult, verbal humans is likely to be influenced by instructions, implicitly or explicitly arranged, whereas that of nonhumans is most likely not so influenced. An important implication is that formal similarities between human and nonhuman behavioral phenomena might be superficial. Important differences might emerge when conditions are changed in particular ways. Instructed behavior, for example, in contrast to behavior directly shaped by reinforcement, is at least partly controlled by the stimulus events that we call instructions and so may be relatively less influenced by current contingencies (Chapters 1, 2, 5, and 10). Furthermore, the response forms evoked by instructions and those forms directly shaped are members of different operant classes. And the classes controlled by instructions may be very complex indeed, describable by terms like *knowledge*. It is not surprising, then, that the effects of various operations are different depending on the kinds of variables that evoked and maintained the behavior. What may be surprising is that the authors of several chapters see such differences between instructed and noninstructed behavior as favoring a cognitive over a behavioral theoretical approach (Chapters 1 and 10). Again, we may agree with critics (e.g., Chapters 1 and 10) who correctly point out that we do not yet have rigorous behavioral analyses of these kinds of phenomena. But again we respond that we see little evidence that other theoretical approaches have generated more effective accounts.

A behavioral analysis can be as superficial and off the mark as any other type of analysis (Ferster, 1972; Michael, 1986). The class membership of similar response forms can be misidentified, and

procedures can be described simplistically. Ferster (1972), for example, has argued that it is a mistake to assume a simple one-to-one correspondence between human token economies and token reinforcement schedules studied with nonhuman animals. He has suggested that token economies work for reasons other than that the tokens are conditioned reinforcers due to having been correlated with primary reinforcers. Tokens likely serve complex stimulus functions within a complex social network of contingencies. These kinds of complexities and their implications are not developed in the reviews of token systems (Chapters 3 and 7). To the extent that these other factors are important, failure to recognize them and their effects will lead to procedures that are less effective than otherwise possible. Furthermore, when the analysis proceeds without due sensitivity to the complex controlling relations that might be operating, the behavior analyst will be unprepared for certain kinds of effects of various operations. Michael (1986), an experienced behavior analyst, stated the point forcefully: "Incorrectly used technical language is worse than commonsense language since it suggests an expertise that is not present, and by implying that the situation is well understood may head off serious attempts to understand it." (p. 16)

Critics (e.g., Chapters 1 and 10) are appropriately sensitive to cases where behavior analysts have equated procedures and behavioral results on the basis of superficial similarities. What is unfortunate is that such critics attribute the incorrect interpretations to the behavioral approach per se instead of to faulty or incomplete behavioral analyses. The unfortunate consequence is that the critics then conclude that a shift toward a cognitive theoretical approach will solve the problem. If, however, superficiality of the behavior analyses or incompleteness of our understanding of behavior principles is the problem, an effective solution would be better training in behavior analysis so as to produce more sophisticated empirical and conceptual behavior analyses (Michael, 1980, 1986).

Acknowledgment of ignorance should be tolerated, even encouraged, in any science. It may well turn out that aspects of human behavior cannot be described comfortably by principles revealed in research with nonhuman animals (Hayes, 1987). But our hunch is that we will be most likely to discover the limits of our behavior principles by focusing our research and analyses sharply on those principles and on critical aspects of human behavior (e.g., Catania, 1980). To do this effectively requires a sophisticated understanding of behavioral principles and the courage to push those principles to their limits in a rigorous, disciplined manner. It is easy to give up in the face of tough cases. But unless the approach is pursued vigorously, we will never know its inevitable shortcomings.

We would hope also that when the need for new principles is documented, some effort is made to establish relationships between the new principles and traditional ones. Sidman's (1986) attempt to relate stimulus equivalence effects to traditional principles through a classification of contingencies is exemplary.

Unless principles are developed systematically, behavior analysis, as a field, may end up as fragmented as psychology as a whole. This would be most unfortunate because one of the special features of behavior analysis has been its systematic approach: our subject matter can be organized within a coherent framework. Doing so may reveal previously unnoticed commonalities as well as significant gaps or anomalies which, in turn, can help workers set priorities for their research efforts. Skinner (1938) made much the same point about the value of a systematic approach to the analysis of behavior:

> The mere accumulation of uniformities is not a science at all. It is necessary to organize facts in such a way that a simple and convenient description can be given, and for this purpose a structure or system is required. The exigencies of a satisfactory system provide all the direction in the acquisition of facts that can be desired. (pp. 44–45)

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